



## Discussion

## Use-novel predictions and Mendeleev's periodic table: response to Scerri and Worrall (2001)

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## ARTICLE INFO

## Keywords:

Periodic table  
Dmitri Mendeleev  
Noble gases  
Use-novel predictions  
Heuristic account  
Ad hoc hypotheses

## ABSTRACT

In this paper I comment on a recent paper by [Scerri, E., & Worrall, J. (2001). Prediction and the periodic table. *Studies in History and Philosophy of Science*, 32, 407–452.] about the role temporally novel and use-novel predictions played in the acceptance of Mendeleev's periodic table after the proposal of the latter in 1869. Scerri and Worrall allege that whereas temporally novel predictions—despite Brush's earlier claim to the contrary—did not carry any special epistemic weight, use-novel predictions did indeed contribute to the acceptance of the table. Although I agree with their first claim, I disagree with their second. In order to spell out my disagreement, I not only revisit Scerri and Worrall's interpretation of crucial historical evidence they have cited in support of the 'heuristic account' of use-novel predictions, but I also criticise the latter on general grounds.

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When citing this paper, please use the full journal title *Studies in History and Philosophy of Science*

## 1. Introduction

In 2001, Worrall and Scerri critically reviewed claims held by philosophers and historians of science (Maher, 1988; Lipton, 1991; Brush, 1996) that the acceptance of Dimitri Mendeleev's periodic table (1869) within the scientific community was strongly influenced by the prediction of novel elements made by Mendeleev based on 'gaps' within the table.<sup>1</sup> The intuition behind these claims has to do with the justification of theories. If evidence *e* temporally precedes hypothesis *h*, which explains *e*, then *h* can always be constructed in such a fashion that it will account for *e*; however *h* would then be ad hoc. If on the other hand, *h* precedes *e* then *h*, so the story goes, takes a high risk of being refuted in case *e* is not observed but instead *e'* is. However, it has turned out to be ridiculously hard to find any historical evidence that would support the thesis of the special epistemic weight of temporally novel predictions. This was first

pointed out by Worrall (1989) in his study of the myth surrounding the impact of the novel prediction of the Poisson spot made by Fresnel's theory of light. Inspired by Worrall, Brush (1989, 1990, 1993a,b, 1994) has confirmed Worrall's claim in numerous studies in various fields of physics. Yet, Brush (1996) thought he had found a case in Mendeleev's periodic table, where novel predictions did play a role (albeit it was one among a number of other factors) in the acceptance of the table.<sup>2</sup> This claim, in turn, was contested by Scerri and Worrall, who argued that the conclusions Brush drew from the historical material were unwarranted. According to them, even Mendeleev's periodic table gives us no reason to believe in the special epistemic weight of temporally novel predictions in the appraisal of theories. Despite wrong-headed criticisms by Barnes (2005) and enduring resistance by Brush (2007), I think Scerri and Worrall have shown almost beyond doubt that novel predictions—contrary to all intuitions—did not extraordinarily contribute to the acceptance of

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<sup>1</sup> See also Shapere (1977), McIntyre (2002) and Niaz et al. (2004); see the latter for a review of the available literature on this topic.

<sup>2</sup> Brush (2007) has re-confirmed this view. I would like to thank the referee for prompting me to weaken a stronger formulation of Brush's position.

the table. However, Scerri and Worrall also claimed that their study gave evidence to the ‘heuristic account’ devised by Zahar (1973) and defended by Worrall (1989). The heuristic account replaces temporal novelty by use-novelty: a fact *e*, explained by *h*, is to be considered novel if *e* has not been used in the construction of *h*, or in the words of Deborah Mayo: (i) *T* entails *e*, but (ii) *e* is not used in the construction of *T*. Mayo (1991) has called this the use-novelty rule (UN).<sup>3</sup>

Since its conception, the heuristic account has faced the charge from psychologism: one actually needs to get into the head of someone in order to establish whether a particular piece of evidence was used in the construction of *h* or not. Worrall (1989), however, has always denied this charge quite vehemently and has claimed to the contrary that the historical research is sufficient for telling us whether the UN rule was violated or not. And yet, we cannot presume that all the relevant construction decisions will be stated explicitly in the publicly available material. We would therefore have also to consult private correspondences and unpublished work in order to establish whether UN is violated or not, that is, whether a particular hypothesis is ad hoc or not. There is then always the chance that material will pop up which is capable of refuting the conclusions reached on UN violation. This is exactly what happened to Zahar’s claim that Mercury’s perihelion did not play any role in the construction of Einstein’s theory of relativity. Glymour and Earman (1978) examined Einstein’s unpublished correspondence with Arnold Sommerfeld which ‘strongly suggests that Einstein did in fact use the known behaviour of Mercury’s perihelion in choosing his field equations for general relativity’ (Brush, 1994, p. 134), flatly contradicting Zahar’s earlier claim. Findings like Glymour and Earman’s cast severe doubt on Worrall and Zahar’s programme. How can we ever be sure whether a particular fact was used in the construction of a particular theory? But even if we did have all the relevant material at our disposal, the whole rather esoteric endeavour of figuring out which facts were used in theory construction is doomed to fail because, as Gardner (1982) points out, normally the unpublished material is not available to the scientific community in the appraisal of theories, which, after all, is at issue. Furthermore, it might well happen that the originator of the theory in question did use a particular fact in the construction of his theory but just fell short of mentioning this (see also *ibid.*, p. 6). And it is not immediately clear that we can always make out from the author’s work whether that was the case or not, without having to rely on the author’s personal reports given about the construction process.

## 2. The heuristic account in its normative guise

In order to give the heuristic account the appearance of a logical account (rather than a psychological one), Worrall has attempted the following re-formulation of the UN rule:

If, for example, *e* is used to fix the value of an initially free parameter  $\lambda$  and thus turn more general theory  $T(\lambda)$  into more specific theory  $T(\lambda_0)$  then, since *e* deductively entails  $T(\lambda_0)$  given  $T(\lambda)$ , there is of course a clear sense in which *e* supports—indeed maximally supports— $T(\lambda_0)$ . Evidence *e* establishes  $T(\lambda_0)$ , given that the more general theory  $T(\lambda)$  has already been accepted. But *e* gives no extra reason to accept the more general  $T(\lambda)$  itself. (Worrall, 2005, p. 817)

That is, although *e* supports the specific theory *T'* (maximally), which had been derived from *T* by fixing a particular parameter with *e* the same evidence (*e*) does not support the general theory *T* (let me call this condition [gen-spec]<sub>negative</sub>). In other words,

*T'* is theory that was modified in response to *e* and [gen-spec]<sub>negative</sub> is thus an attempt to discount ad hoc manoeuvres. Worrall has illustrated this condition with two examples (Worrall, 2002, 2005; Scerri and Worrall, 2001).

The general wave theory of light by Fresnel does not specify the wavelength of any particular kind of monochromatic light. The general theory of light characterizes the observable fringe separation as a one-to-one function of the wavelength (i.e.  $X/X^2 + D^2)^{1/2} = \lambda/d$ , where *d* is the distance between the two slits, *D* the distance from the two-slit screen to the observation screen, and *X* the fringe separation. In order to work out a particular wavelength given a particular light source (e.g. sodium arc) we would measure the fringe separation (*e*), plug it in the above equation of the general theory (along with the appropriate values for *D* and *d*) and would thereby obtain the wavelength for light from a sodium arc. Worrall argues that by fixing the initially free parameter in the general wave theory of light *T* to a particular value by using the experimental evidence of the particular fringe separation we have produced a specific wave theory for sodium light *T'*. Now, Worrall argues that although experimental evidence *e* fully or maximally supports theory *T'*, *e* does not support *T*! However, the prior acceptance of the general wave theory *T* is a necessary precondition for *e* to support *T'*, because *T'*, after all, is just a specific version of *T*. This scheme Worrall also sees at work in so-called creation-science. The fossil record is explained by the creationists by conjecturing that what might look like the remains of once living beings in fact are just due to God’s playfulness and are merely bone-like structures painted in tar pits, desert sands and rocks by God (Worrall has called those explanations ‘Gossean’ explanations in reminiscence of the English naturalist Philip Henry Gosse (1810–1888), who is perhaps best known for his attempt to reconcile Creationism with the fossil records.) Surely, no-one in their right minds should be compelled to prefer the ad hoc, highly artificial Gosse-explanation over Darwin’s theory, which explains the fossil record naturally and without having to resort to any of those fanciful manoeuvres. However, the point Worrall wants to make here really is that one has already to accept creationism before one can give any credence to the Gosse-version of it. Once one has accepted creationism, fossils do lend support to the Gossean version of creationism but not to creationism itself (by [gen-spec]<sub>negative</sub>). What distinguishes this case of creationism and its Gossean version from the general and specific wave theory of light discussed above and which makes the former a case of UN-violation (i.e. ad hoc) and the latter legit, according to Worrall, is captured by the following two exceptions to his [gen-spec]<sub>negative</sub>, which allow that the evidence which supports the specific theory *T'* indeed also supports the more general theory *T*, from which *T'* was derived (let me call these exceptions [gen-spec]<sub>positive</sub>):

- (i) cases where general theory *T* plus “natural” auxiliaries entail *e* and
- (ii) cases, where *T'* makes ‘independently testable (and observationally verified) prediction[s]’, which are different from the evidence, which was used in constructing *T'* out of *T*. (Worrall, 2005, p. 818)

Whereas the general wave theory of light entails straightedge diffraction, creationism does not make any successful predictions about the fossil record without any artificially constructed ad hoc auxiliaries, such as the Gossean hypothesis about fossil records (condition i). Whereas the specific version of the wave theory of

<sup>3</sup> Most recently, the criterion of use-novelty has been used by Psillos (1999) and others in defending scientific realism against the challenge of the Pessimistic Meta Induction. For a criticism of Psillos’s use of the novelty criterion from another perspective see, for example, Doppelt (2005).

<sup>4</sup> Although Worrall does not say this explicitly, presumably both condition (i) and (ii) have to be satisfied jointly in order for evidence *e* to support both *T* and *T'*.

light implies *other* predictions like fringe separations in different experiments, the Gossean version of creationism does not gain any independent support (condition ii).<sup>4</sup>

### 3. A problematic case: the periodic table

Most recently, Scerri and Worrall (2001) claimed the validity of the heuristic account in their impressive case study of Mendeleev's periodic table. Scerri and Worrall made clear that Mendeleev's successful prediction of new elements, regardless of our strong intuitions against it (see Introduction) at the time was *not at all* seen as an extraordinary success of the theory, which would have to be located above the accommodation of already known elements in terms of theory appraisal.

There is one part of Scerri and Worrall's paper, which Worrall (2005, p. 825), in his reply to Barnes's (2005) criticism,<sup>5</sup> calls the 'truly central part of our analysis'. In this part, Scerri and Worrall discuss the discovery of the element argon, which the periodic table in its present form could not accommodate. In order to fit argon into the periodic table, the latter had to be adjusted accordingly. *Prima facie*, this move seems to violate the UN rule of the heuristic account, i.e. this move seems to be an instance of [gen-spec]<sub>negative</sub>, but Scerri and Worrall have advertised this episode as a victory of the heuristic account.

Argon, the first element from the group of noble gases to be discovered constituted an anomaly to Mendeleev's periodic table. According to Worrall and Scerri argon 'had to fit in the table somehow, and an atomic weight of 40 meant it did not fit' (Scerri and Worrall, 2001, p. 444).<sup>6</sup> After several attempts at somehow resolving the anomaly (among them the supposition that the discovered argon was not mono-but rather diatomic, resulting in half the atomic weight), Ramsey, one of the discoverers of argon, proposed to create a new group of noble gases, where argon and the recently discovered helium could be fitted in. The manoeuvre of inventing a new group within the table in order to accommodate an observation clearly looks ad hoc and seems to satisfy Worrall and Scerri's ad hoc criterion of [gen-spec]<sub>negative</sub>. (In the terminology of their joint paper, this manoeuvre falls under the category of an accommodation<sub>2</sub>):<sup>7</sup>

At first sight, the accommodation of argon and helium by inventing a new group looks exactly like the sort of ad hoc accommodation<sub>2</sub> that we insisted ought to carry less evidential weight. Surely inventing a new group for these elements is exactly a case of 'writing already known phenomena into' a pre-accepted theory without any independent testability? (Ibid., p. 445)

Yet, Scerri and Worrall go on to call this judgement 'deceptive', the reason allegedly being that this particular ad hoc accommodation led to accommodations<sub>1</sub> (i.e. [gen-spec]<sub>positive</sub>):

The atomic weights of the four [sic!]<sup>8</sup> newly discovered noble gases have to be such that each one would fit into a particular

space in each successive period of the table. That is, each of these atomic weights had to be intermediate between two other elements in each period. In addition, this insertion of the four [sic!] new elements had to result in all of them lying vertically below one another in the newly created group. *These are stringent (and ultimately empirically based) constraints*: it is perfectly conceivable that there was no way of placing the noble gases into the table that simultaneously satisfied all those constraints. *In effect, creating a new group for the noble gases leads to a new series of predictions (in the atemporal sense) about already known analogies between elements.* (Ibid., p. 446; my emphasis)

Notice that Scerri and Worrall's, argument as to why the accommodation of the 'newly discovered elements' was an accommodation<sub>1</sub> rather than an accommodation<sub>2</sub> really consists of two parts instead of just one. First, they say that the table imposed constraints, which the newly discovered noble gases had to satisfy. These constraints (basically, ascending atomic weight from left to right and from the top to the bottom) are largely the result of the ordering criterion of the table invented and deployed first by Mendeleev. These constraints, contrary to Scerri and Worrall's remark that they are 'empirically based', are intrinsic to the table and don't exist 'outside' of it. But more importantly, as we shall see in a moment, these 'stringent' constraints were not capable of accommodating<sub>1</sub> helium and argon. Secondly, Scerri and Worrall assert that the introduction of the new group was *justified* by the 'already known analogies between elements'. Scerri and Worrall are not very clear about what exactly they mean by these analogies but I take it that they mean things like similarities in the reactive behaviour of those elements in experiments, their physical properties and so on. Yet, although these analogies might indeed have helped Mendeleev as a sort of heuristic guide or secondary criterion, as Scerri and Worrall do not fail to mention (ibid., pp. 437–438), those analogies surely were not sufficient for ordering the elements. They themselves quote Mendeleev saying that

it is easy to fall into error in the formation of the groups because *the notions of the degree of analogy will always be relative, and will not present any accuracy or distinctness.* Thus lithium is analogous in some respects to potassium and in others to magnesium; or beryllium is analogous to both aluminium and magnesium. (Mendeleev, 1891, p. 15; my emphasis)

That is, *some* analogies could be found between almost any of the known elements. Analogies are therefore insufficient for ordering the elements. It was Mendeleev's genius to understand this and to raise the atomic weights of the elements into the status of the principle ordering criterion underlying his table. Since those analogies were simply too 'soft' in order to set up the table, they likewise cannot serve as a proper test for the table. It would therefore be going too far to claim, as Scerri and Worrall do, that the newly introduced group of the noble gases was justified by these 'soft' analogies.

<sup>5</sup> Barnes (2005) erroneously interpreted Scerri and Worrall (2001) as defending (temporal) predictivism. Worrall (2005) and Scerri (2005) set this straight.

<sup>6</sup> In a paper by Carmen Giunta entirely dedicated to the discovery of argon, one can read that 'An atomic weight of just under 40 would have squeezed argon between potassium and calcium, between the alkali metals and alkaline earths, with which it had no [chemical] properties in common' (Giunta, 2001, p. 113). Rayleigh, the collaborator of Ramsay, said somewhat desperately: 'The facts were too much for us; and all that we can do now is to apologize for ourselves and for the gas' (cited by Giunta, ibid.).

<sup>7</sup> Let us note that Scerri and Worrall (2001) use a different terminology from the one discussed above: 'call cases in which the fact happened to be already known before a theory entailed it, but in which no feature of the theory was "read off" the fact, a case of accommodation<sub>1</sub>. And all cases in which, on the contrary, the fact was both known and used in the construction of the theory that entails it, cases of accommodation<sub>2</sub>' (Scerri and Worrall, 2001, p. 424; original emphasis). That is, what we have called [gen-spec]<sub>positive</sub> above corresponds to accommodation<sub>1</sub>, and [gen-spec]<sub>negative</sub> to accommodation<sub>2</sub>. I shall use both notions interchangeably below.

<sup>8</sup> It is not quite so clear to which 'four' elements Scerri and Worrall are actually referring here. Prior to the quoted passage, only argon and helium are being mentioned, with the remark that helium was known already in 1868—even before Mendeleev published his first table. Since both helium and argon were known before the new group was introduced they of course qualify as accommodations (potentially in both senses of Scerri and Worrall's use; see n. 7). After the quoted passage Scerri and Worrall mention neon as the 'third' noble gas (discovered in 1898, a year after Ramsey's proposal of the new group) which (together with the other noble gases later to be discovered, krypton and xenon) qualify as temporally novel predictions, as Scerri and Worrall concede (2001, p. 446). Thus, in order to make sense of the above mentioned passage one has to presume that Scerri and Worrall are actually referring only to two accommodated elements (helium and argon) rather than to four.

<sup>9</sup> Scerri and Worrall draw attention to this very nicely in Section 4.

On the other hand—and this brings us back to the first point touched upon above—it also has to be noticed that the atomic weight criterion alone could not have possibly done the job.<sup>9</sup> First of all, although Mendeleev claimed that he predicted the atomic weight value of yet undiscovered elements by interpolating the values of adjacent elements, this was not always the case (in particular for his most cherished predictions). Rather, as Scerri and Worrall point out, Mendeleev seemed to have had (most likely implicit) ‘extra assumptions’ and heuristics, which he must have used when departing from the ‘simple method of interpolation’, which he never disclosed. Even more importantly, the atomic weight criterion is of course insufficient for breaking the ascending order of elements into groups and therefore for constructing a table out of a series. Here, secondary criteria like reactivity behaviour and physical constitution *had* to kick in. On rare occasions, these secondary criteria even *overruled* the atomic weight criterion. And this is exactly what happened in the case of argon. The atomic weight of argon is higher than the atomic weight of potassium, which lies in the first group of the next period from where argon was finally to be located. In fact, the atomic weight of argon and potassium is one instance of the few ‘atomic weight inversions’, which caused some trouble before the ‘true’ ordering criterion of atomic number was proposed by Moseley in 1913. But because Sir William Ramsey (1852–1916) must have regarded the analogies between argon and helium as more important than the ‘unfitting’ weight of argon, he disregarded the atomic weight criterion in this particular case, enabling him to set up the new group of noble gases.<sup>10</sup> In other words, the ‘stringent constraints’ of the periodic table (in the form of the atomic weight criterion) to which Scerri and Worrall refer in the quote above were in fact violated by argon. This leaves only helium as an instance of accommodation,<sup>11</sup> which is rather meagre evidence for the group of noble gases. And yet, one might want to say that the new group was justified by the later discoveries of the other noble gases krypton, neon and xenon, which were *predicted* by the new group. But Scerri and Worrall themselves have ruled out this option:

There is, as far as we can tell, again no support in the historical record for the idea that the prediction of neon played any particularly ‘crucial’ role here or that it counted for any more than the ‘accommodation’ of argon—if anything, the contrary. (Scerri and Worrall, 2001, pp. 446–447)<sup>12</sup>

#### 4. General problems with the heuristic account and the condition of independent testability

Apart from the worries, discussed in the last section, about the particular example of the periodic table, quoted by Scerri and Worrall (2001) as evidence for the heuristic account, there are several general difficulties with the heuristic account. Although Worrall has strongly emphasised that his account is to be taken as a normative and logical account and not as a psychological one, whereby the particular motivations to construct a theory do not have to be considered, I don’t think Worrall succeeds in making his case. After all, condition (ii) of [gen-spec]<sub>positive</sub> still requires us, besides the independent testability of T’, to know which particular evidence was used in constructing T’ out of T. But even if one were to ignore this point, the criterion of independent testability of condition (ii) is highly problematic for the following reasons. First of all, condition (ii) of [gen-spec]<sub>positive</sub> does not seem to add anything to our intuitive understanding of ad hoc hypotheses. Is the

very concept of an ad hoc hypothesis not defined customarily as something, which merely saves the phenomena, without having any other justification? (see e.g. Leplin, 1982) What is it that the heuristic account adds here? Second, condition (ii) is playing directly into the hands of Deborah Mayo (1991, 1996) who has claimed that what really underlies Worrall’s heuristic account and the UN rule is a criterion of severity of test. According to Mayo, the UN rule just ‘furthers the aim of guaranteeing genuine and avoiding spurious tests’ and that ‘what lies behind the intuition that novelty matters, is the deeper intuition that severe tests matter’ (ibid., p. 526). Although Worrall (2002) has stated that he disagrees with Mayo, a counter-argument against Mayo from his part is still pending. In the way Worrall has presented his account most recently, it does seem as though the emphasis is laid upon testability and not on the construction of hypotheses, as it was originally. There are two more arguments, which can be made against condition (ii) of [gen-spec]<sub>positive</sub>. One of them is empirical, the other two of a theoretical nature. Laymon (1980) has pointed out that in order to apply the independent testability criterion, one needs to be able to distinguish between different experimental types a and b for the original theory T and the amended theory T’. Laymon showed that in the case of the ether theory amended by the Lorentz–Fitzgerald contraction hypothesis, this cannot be guaranteed. Laymon concludes that:

Given our difficulties in rendering unproblematic judgments about experimental type, we should be prepared to entertain the thesis that *independent testability is a red herring* and that other considerations are paramount in appraisals of *ad hocness*. (Laymon, 1982, p. 281)

Lastly, there is a delicate time dimension to condition (ii): at what point in time can we say conclusively that T’ has been confirmed independently? How long do we have to wait for until we can decide whether T’ is an illegitimate ad hoc version of T or a legitimate one? To a defender of T’ the option of resorting to the independent testability criterion is always open. One can always claim that although at time t no independent evidence for T’ is available, it will be at time t + n. The independent support criterion then itself becomes some form of ad hoc manoeuvre.

#### 5. Conclusion

In this paper we have seen that, contrary to what Scerri and Worrall (2001) claim, the heuristic account is not apt to account for the acceptance of the group of noble gases introduced by Ramsey into the periodic table. We also saw that the heuristic account, apart from the classical charge of psychologism, in its normative guise, amended by the condition of independent testability, faces severe difficulties. Are there any alternatives to the heuristic account for dismissing ad hoc hypotheses? I am prepared to argue that rather than trying to dismiss ad hoc-ness, it might be more fruitful to choose a ‘positive approach’ and assess theories on their degree of ‘naturalness’ (Schindler, *In preparation*).

#### Acknowledgements

I would like to thank my PhD supervisor Steven French and an anonymous referee for valuable comments. I am grateful to the Deutsche Akademische Austauschdienst (DAAD) for funding my PhD project of which this paper is a part.

<sup>10</sup> This explains why Mendeleev did not introduce the new group although he seemed to have suspected a new group (see Giunta, 2001). However, Mendeleev welcomed Ramsey’s move but thought that the weight inversion of argon and potassium (and the other instances in the table) were due to measurement errors (see *ibid.*).

<sup>11</sup> See n. 8.

<sup>12</sup> Krypton was discovered in May, neon in mid June, and xenon in September 1898. Scerri and Worrall (2001) do not even mention krypton and xenon in their paper, but I think one can safely assume that they would not want to assign more epistemic weight to their prediction than to the prediction of neon.

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